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THE IMPACTS OF CREDIT ON VILLAGE ECONOMIES

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Abstract

This paper evaluates the short-term impact of Thailand's 'Million Baht Village Fund' program, among the largest scale government microfinance initiative in the world, using pre- and post-program panel data and quasi-experimental cross-village variation in credit-per-household. We find that the village funds have increased total short-term credit, consumption, agricultural investment, income growth (from business and labor), but decreased overall asset growth. We also find a positive impact on wages, an important general equilibrium effect. The findings are broadly consistent qualitatively with models of credit-constrained household behavior and models of intermediation and growth.

1 Introduction

We study the impacts of Thailand's Million Baht Village Fund Program, a large-scale government intervention that acted as an exogenous injection of potential funds into 77,000 heterogeneous Thai villages¹ The program was among the largest scale government microfinance initiative of its kind. Each transfer of one million baht (about \$24,000) was used to form an independent village bank for lending within the village. Every village, whether poor or wealthy, urban² or rural, was eligible, and all sixty four villages for which we have panel data did indeed receive the funds. The size of

¹The Thai program involves approximately \$1.8 billion in initial funds, or about 1.5 percent of Thai GDP in 2001. This injection of credit into the rural sector is much smaller than Brazilian experience in the 1970s, which saw a growth in credit from about \$2 billion in 1970 to \$20.5 billion in 1979. However, in terms of a government program implemented through village institutions and using micro-lending techniques, the only comparable government program in terms of scale would be Indonesia's KUPEDDES village bank program, which was started in 1984 at a cost of \$20 million and supplemented by an additional \$107 million in 1987. (World Bank, 1996)

²The village (*moo ban*) is an official political unit in Thailand, the smallest such unit, and is under the sub-district (*tambon*), district (*amphoe*), and province (*changwat*) levels, respectively. Thus, "villages" can be thought of as just small communities of households that exist in both urban and rural areas.

the transfers were substantial. Across our sample, the transfers averaged twelve percent of total annual income in the village economies, and forty-one percent of total short term credit flows.

We view each of these transfers as a smaller, though substantial, version of the broader increases in financial intermediation, which have been well studied at the macro-level.³ A criticism of some of this literature is that intermediation is endogenous. But here, for us, village capitalization has an important degree of exogeneity making them “test tube”-like experiments for studying phenomena important to macro-economics, including general equilibrium effects. More specifically, two crucial elements of the structure of the Million Baht program gave the transfers this (plausible) exogeneity. First, the program was a rapidly introduced “surprise” policy initiative. In November 2000, the Thai Parliament was dissolved, and by January 2001, the populist Prime Minister Thaksin Shinawatra was elected. The new policy was implemented quite rapidly. None of the survey villages had the village banks in the 2001 data, but all had them by the 2002 data. Second, there is strong variation in the intensity of the credit injection in the cross-section of villages. Specifically, each village received the same amount – one million baht – regardless of the population of the village, so smaller village economies received a relatively more intense injection of credit. For example, the million baht transfer injection averaged 27 percent of income for the lowest quintile (i.e., smallest) village economies, and less than 2.5 percent for the top quintile (i.e., largest) village economies.

We use an interaction of the year of the program introduction and the number of households in a village as instrument for the amount of credit received and the probability of receiving credit. We believe that this variation in credit driven by variation in the inverse number of households per village in the post-program years is exogenous. *A priori* we know that most of the variation in inverse number of villages is among small villages (i.e., between 50 and 250 households), and indeed ex post our results are robust to the exclusion of the few large (and very small) villages that are not in this range. Second, villages are geopolitical administrative units, and it is not uncommon for villages to be split for administrative purposes. Finally, while the crucial instrument is the additional interaction of inverse village size during the program years, village size itself is not significantly related to the levels or growth of credit or other the outcome variables. That is, after controlling for household characteristics, villages look very similar in terms of their levels and growth rates until the program is instituted.

³Earlier influential work by King and Levine (1993) establishes correlations between growth and private sector intermediation. Rajan and Zingales (1996) is an attempt to establish causality. Aghion et al (2005) models the non-linear relationship between financial intermediation on convergence. Townsend (2009) gives a very detailed analysis of the Thai experience of growth with increased financial intermediation.

It is also important to keep in mind that each village we consider is in many ways its own small economy. The village economies are open economies, but not identical and not entirely integrated with one another and the rest of the broader economy (nearby provinces, regions, etc.). It matters where a person lives. There is substantial variation in institutional and market arrangements across villages (Townsend, 1995). Certainly informal borrowing and lending within the village is more common than across village lending, and there is cross village variation in interest rates and the amount of credit.⁴ Even labor markets are not entirely integrated with local wages varying considerably across villages.⁵ Finally, risk sharing may vary. The household-specific fixed effects we use attempt to control for much of this heterogeneity, but because village are small (quasi-open) economies, we anticipate movements in quantities and prices that vary with the size of intermediation.

The Townsend Thai dataset we use has unique advantages. It contains seven years (1997-2003) of panel data on 960 households across 64 villages in four rural and semi-urban provinces of Thailand. These data include information on education, assets and investment, income, borrowing and saving through various forms, consumption, occupation, and household composition, for example. The first five years of data give us a “before” picture of the environment, while the final two years give us the ability to look at the effect of the program on levels and growth rates of relevant outcome variables. The relatively short “after” horizon gives us a window for examining the impacts of credit on villages, at a time when these impacts were still localized. (Indeed, neighboring village impacts constructed using GIS techniques are small and typically insignificant.) A smaller monthly panel with only 16 villages has separate information on labor supply and wage rates.

Our regressions use short-term village fund credit as a measure of treatment and assess its impacts on households, including those running small businesses. The major impacts we examine are the effect of the new village institutions on (other and total) credit, saving and investment decisions, consumption, asset growth, income and income sources, wage rates, and business enterprise. We run two-stage regressions with household-specific fixed effects using both levels and changes in levels as the dependent variables. Additionally, we control for observable household characteristics (i.e., household composition, age and education of household head) and add year-specific dummy

⁴The ratio of the number of loans to relatives within vs. outside of the village is 2:1, for non-relatives this ratio is 3:1 and interest rates are much lower on within-village loans. Small loans are less likely between households in different villages. (Kaboski and Townsend, 1998)

⁵For each village in Thailand, we have a reported average wage in the village from the Thai Community Development Department. Among the four provinces (*changwats*) we examine, the within-province coefficient of variation in average daily wage across villages ranges between 23 and 41 percent.

variables.

1.1 Findings in Light of Theory

Our analysis is motivated by two broad classes of theories on credit constrained environments. The first class of models is the buffer stock savings model of households in the presence of borrowing constraints and income uncertainty, modified to include investment. The households in Thai villages we study faced relatively low average returns on liquid savings relative to returns on capital and face limited borrowing. Default is not uncommon (average credit in default is about 12 percent of average income). Our companion paper, Kaboski and Townsend (2008), uses the same Thai Million Baht intervention to estimate an explicit structural model of household decisions: consumption; low-yield liquid savings held as a buffer against income shocks; high-yield, illiquid, and indivisible investment projects; and default. In contrast, this paper uses reduced form regression methods to delve more deeply into the details of consumption, investment/business, and credit decisions.

Many of the findings here are broadly consistent with such an interpretation.

First, the availability here of credit increased total borrowing, and so crowding out of or substitution away from other sources was not a major issue. Indeed, we cannot reject the null hypothesis that credit increased one-for-one with the injection of available credit. At the same time, average interest rates on short-term credit did not fall, but rather rose slightly. This can be viewed as evidence that households were originally credit constrained, since credit increased even though interest rates did not fall. Thus, similar to Banerjee and Duflo (2004), we see that households are not merely substituting toward lower cost credit or expanding borrowing in response to lower borrowing costs. Among the purposes and types of credit that increased were credit for consumption, credit for agricultural investment, and credit from the agricultural bank and perhaps commercial banks as well.

Second, total consumption increased substantially, perhaps one for one with credit, which indicates credit constraints are particularly binding in consumption decisions. The magnitude of such an increase in consumption could not be explained in a permanent income model. Credit would at most have a wealth effect, and consumption responses would be bound by the interest on this wealth effect (e.g., seven percent). The results are consistent with buffer stock models, however, where the ability to borrow has large effects on consumption both by impacts on the consumption behavior of currently constrained borrowers and also on those with the potential to borrow in the

future.⁶ The composition of consumption increases is also of interest. Grain, tobacco, ceremony, and educational expenditures were stable, but credit increased expenditures on household and auto repair, fuel, meat, dairy goods, and alcohol. The more typically income elastic components of consumption or those with an intertemporal element (like repairs) responded the most to credit. The increase in fuel usage and auto repairs is consistent with Karlan and Zinman (2008)'s findings for payday loans in South Africa.

The prevalence of households in default rose in the year of repayment, and households defaulted on short-term credit. In the bufferstock model in our companion paper, increased borrowing, particularly for consumption, can increase the probability of future default. (The model also produces a decline in default in the initial year of the program, which can be observed using year-specific coefficients.) The use of informal credit was apparently unaffected by the program, however.

The second broad class of models motivating our analysis are models of macro-intermediation, entrepreneurship and growth (e.g., Lloyd-Ellis and Bernhardt, 2000, Greenwood and Jovanovic, 1990, Banerjee and Newman, 1993, Buera and Shin, 2008, and Buera, Kaboski, and Shin, 2008). Such models have been shown to perform relatively well in fitting the long run Thai growth experience (see Gine and Townsend, 2004, Jeong and Townsend, 2003, and Townsend and Ueda, 2006, forthcoming), some with endogenous financial deepening and some with an exogenously expanding credit sector. In these models, improvements in intermediation on the extensive and/or intensive margin can spur business or agricultural investments and growth in business income. Improvements in intermediation can also have indirect impacts via changes in wage rates or relative prices of tradables and non-tradables.

The implied connection between access to finance, entrepreneurship, and growth is often a central motivation for microfinance programs as poverty alleviation interventions. Microfinance programs typically cater to poor people who lack access to other forms of intermediation in the hope that the poor are financially constrained and have high returns to investment. Women, in particular, are often targeted under the belief that they have less access to credit, lower outside options in the labor market, and therefore the highest returns to private entrepreneurship.

The results here under a quasi-experimental intervention are mixed with regards to the predictions of these models. On the one hand, we indeed measure significant increases in income growth

⁶The fact that informal credit and household lending did not respond, however, indicates that relending to non-borrowers, as in Angelucci and De Giorgi (2006), is not a major issue.

and a change in the composition of income as a result of the intervention. As the models would predict, business and labor market income tended to increase, while agricultural income from crops other than rice tended to decline. We also find increases in total payments to labor. On the other hand, business and labor income did not seem to be driven by the extensive margin of investment and business starts themselves. To the contrary, we find no change in business starts or business investment, and assets actually decline in response to the program. We do see an increase in the frequency of agricultural investments, but a reduction in the use of fertilizer, and, again, agriculture overall declines as a fraction of income.

A few potential explanations are suggested by the data. First, the structural model in our companion paper provides a quantitative explanation for why impacts on investment levels may be difficult to discern in the sample size we analyze. Investments are lumpy and infrequent, and so the distribution of realized investments is highly skewed. Second, households report both increased labor income and higher payments to outside laborers in response to the program. Perhaps credit was most useful as working capital, allowing businesses and farms to hire more laborers and potentially use more intermediate inputs. That is, perhaps it is the intensive margin, and access to working capital, rather than fixed entry costs that most constrain households in their business activities. McKenzie and Woodruff (2006) offer complementary evidence that fixed costs in Mexico are negligible, yet they find high average returns. Their experiments in Sri Lanka (McKenzie and Woodruff, 2008) also find high returns to increases working capital among entrepreneurs. A third possibility is that credit offers consumption-smoothing, cashflow management, and/or limited liability, which, for a given level of investment, can change the composition of investment and labor decisions toward higher risk but higher yield sources of income a la Greenwood and Jovanovic (1990) and Braverman and Stiglitz (1986). Indeed, the buffer stock model of our companion paper, predicts a decline in low return liquid assets (along with a move toward high return investment). Evaluating this conjecture on the composition of investment is difficult, however, since measuring second moments of returns on disaggregated investments is non-trivial.

Another implication of the macro-intermediation, entrepreneurship and growth models is the general equilibrium effect of intermediation on wages. Intermediation can lead to an increase in wages because of increased investment and productivity increases from a higher quality pool of entrepreneurs, which lead to an increased demand for labor. Gathering experimental evidence for such mechanisms is clearly difficult, but the sheer scale of the Thai Million Baht intervention together with labor markets that are fairly segmented across villages allow us to discern impacts

on wages. We find that wage rates increased for some occupations (general non-agricultural labor, construction in the village, and other), but not for occupations outside of the village.

1.2 Existing Literature on Microfinance

A microfinance initiative is a natural financial intervention to examine because of its policy relevance. Over the past twenty years, “microfinance” has become a key word among both researchers and policymakers in development economics. Initiatives are sponsored by a variety of organizations, including the World Bank, United Nations, USAID, national governments and many charitable NGOs. There are an estimated 120,000 microfinance initiatives worldwide, and the number is growing. A growing literature has arisen to evaluate such programs.

The advantages of this study relative to previous work on microfinance interventions are essentially four-fold. First, the program is unique because of size of the intervention is large and its consequent policy importance. A key policy question in the evaluation of smaller microfinance programs is the extent to which they can be scaled up for larger scale poverty reduction, or whether large scale increases in credit availability might hamper the programs (Duflo, 2004, World Bank, 2004). Second, as stated earlier the size of the intervention and the segmented credit and labor markets yield general equilibrium effects both within, and potentially beyond, the village economies.⁷ General equilibrium impacts of credit programs may be important for large scale programs, and identifying these impacts at the micro-level also gives potential insights into the micro-mechanisms behind macro-theory. For example, enhanced credit may increase investment and employment, potentially moving the wage rate. Microevaluations have great difficulty identifying general equilibrium effects. Third, we have data on households and small enterprises, and the relevant variables necessary to consider potential channels of impact in an environment of local, household-level investment and occupational choice decisions. Finally, the program design produced a convincing, exogenous instrument for evaluation. Our exogeneity has both a cross-sectional and timing element, which is important since impacts may vary over time.

Of course, our paper contributes to an existing literature that includes many of four advantages above, though not simultaneously. Karlan and Zinman (2008) study a true controlled experiment in which a financial institution randomized loan decisions on consumer loans to wage-earners. Pitt and Khandker (1998) study the Grameen Bank, using cutoff participation requirements as an in-

⁷In principle, aggregate (economy-wide) general equilibrium effects would not be identified by our methods. However, since the general equilibrium impacts we find do not seem to extend to neighboring villages (see Section 3.3), we don’t think that general equilibrium impacts at an even wider scale are a major issue over time span we examine.

strument. They have a cross-section, larger than ours, with four outcomes: labor supply, child schooling, female assets, and expenditure. The amount borrowed is quite large relative to expenditures per household. Pitt et al. (2003) studies the same program, but examines biometric health outcome measures. Burgess and Pande (2005) also study a big program, but it is an expansion of banks over twenty years differentially across regions in India. Their outcomes are macro level poverty headcount and wage data. Coleman studies much smaller NGO lending in Thailand using a smaller dataset of about 500 people, but with a great variety of variables. He has a set of villages with programs and a set that will receive them in the future. This is a fairly good control, but there is no exogeneity in the timing of how long the program has been used. He has a panel with four survey rounds in one year, so he examines high frequency but only short-term effects. Gertler et al. (2003) study BRI in Indonesia to see if microfinance helps insure against shocks to health. They have an instrument with less clear exogeneity (proximity to financial institutions), but also a fairly large panel data set (the IFLS). Banerjee and Duflo (2003) study firm’s borrowing from banks but not household borrowing. Aportela (1998) looks at the expansion of bank branches and argues it is exogenous. In any event it is a smaller expansion, and he looks only at savings behavior. Finally our results complement the results of Banerjee, Duflo, Glennerster, and Kinnan (2009), who use experimental data in India. They find sizable income effects on owners of existing businesses but increases in consumption for households not in business.

Clearly, the exogeneity of our instrument (the inverse number of households in a village interacted with program years) is a critical argument in our analysis. We present a priori justification for its exogeneity in Section 2, which also discusses the program and data in more detail. Section 3 lays out our methods, explicitly states our exogeneity assumption, and gives *empirical* support for the exogeneity of the instrument. Section 4 then presents the results, while Section 5 concludes and gives direction for future research.

2 Description of Program and Data

We provide an overview of the Million Baht Village Fund, including its quasi-experimental implementation, and then describe the data.⁸

⁸This overview is based on data from the institutional panel data set, as well government materials and informal interviews of village funds committee members, Community Development Department (CDD) officers, and Bank for Agriculture and Agricultural Cooperatives (BAAC) officers and administrators in March, 2002. BAAC administrators were interviewed in Bangkok, while three branch officers, a CDD officer, and six village fund committees were interviewed in Buriram, Chachoengsao and Chiangmai.

2.1 Overview of Million Baht Program

The fund was a key program in Prime Minister Thaksin’s election platform. The primary hope was that the money would be a revolving, self-sustaining fund to be used for investments in occupational development, employment creation and income-generating activities. The official stated objective for the program was to improve the economic and social status of villagers, and to enable villages to be less dependent on government aid in the future. It was also promoted as an attempt to reach the underprivileged, develop a decentralized grass roots approach to growth, and link communities with government agencies and the private sector.

The program was funded by the central government. While it is difficult to know precisely how the program was funded, it clearly entailed a substantial transfer from Bangkok to rural areas in line with the populist goals of the government. For example, the households in the rural areas pay little to no taxes.

The transfers were given to the villages with both carrot and stick provisions to encourage sound management and repayment of loans. The stick involved telling villages that if the funds were abused or the village institutions failed, they would be offered no further assistance, and even other sources of government funding would be cut off.⁹ The carrot was the promise to turn the successful village funds into true village banks. That is, the Thai Bank for Agriculture and Agricultural Cooperatives (BAAC) later offered loans to villages that receive their highest rating. Their hope was that the funds would assist them in providing financial access to rural communities. These additional loans were first made available after the period of study in this paper, however.

2.1.1 Organization and Founding

The program was jointly administered by multiple government agencies. In the rural and semi-urban areas we study, the BAAC received the initial money transfer and held both the lending and savings accounts for the village funds.¹⁰ Officers from the Community Development Department provided oversight and guidance, as they do with other village funds. Local teaching colleges were in charge of conducting audits of the village funds as well as an evaluation of the funds and member households. These audits are in addition to the BAAC’s own fund ratings mentioned above.¹¹

⁹This threat was not completely credible, which is especially clear since Thaksin is now deposed, but based on interviews it seemed to at least be an important issue to villagers.

¹⁰Each village fund holds two accounts, the first for receiving the million baht transfer and the second for holding member savings. When a loan is granted by the village fund, the member takes a form signed by committee members to the BAAC, and the loan amount is transferred from the fund account to the individual account.

¹¹We, the authors, tried to assist BAAC officials in the development of this rating system.

In order to receive funds, villages needed to form committees, develop policies¹², submit an application/proposal for the village fund, and have the proposal evaluated¹³ and accepted. The vast majority of village households became members of the village funds and village funds averaged 94 members.¹⁴ The committees were selected democratically by the villagers at a village meeting, with regulations set up to ensure fairness of these elections.¹⁵

Although a federal program, the village funds themselves are only quasi-formal, in the sense that they have no building or facility and no employees.¹⁶ They are administered at the village level by a committee¹⁷ elected by the village and by occasional meetings of all villages. Such quasi-formal village institutions are typical in Thailand (see Kaboski and Townsend, 2005). One villager is appointed as an accountant/bookkeeper, and the accounting is fairly detailed, including dated records of all loans, payments, deposits and withdrawals.¹⁸

2.1.2 Policies

Some savings and lending policies were stipulated, while others were set by the villages themselves, often based on the suggestions from printed materials or suggestions from CDD officers.

¹²Government agencies provided villagers with informal advice and manuals describing the goals, procedures and regulations of the village funds. In addition, the appendix contained an example of the policies of a village fund. Although these policies were shown as an example, from interviews, it appears that many committees felt that these suggested policies were fixed regulations for all funds.

¹³The applications in our survey villages were submitted to the BAAC and evaluated first by an district (*amphoe*) level sub-committee with final approval from the national fund committee. The evaluation criteria included: the selection of the fund committee; the qualification of the fund committee including its knowledge, experience and management ability; the policies and regulations of the fund; the extent of participation of villagers and members in the funds management; and the compliance with fund regulations.

¹⁴Any adult could be a member, so many households had multiple members. The primary membership criteria for most institutions was to live in the village. For those households that weren't members, they typically did not want to borrow and two reasons were often given: either the households were wealthy and did not need the money or wanted to leave the funds for poorer households, or the households were poor and did not want to get into more debt.

¹⁵The village meeting required 75 percent of households in the village for a quorum. By regulation, the committee needs to consist of 9 to 15 villagers, with half of them women. Requirements were that committee members be at least 20 years old, have lived in the village for at least two years, be a person of good character (e.g. no gamblers or drug users), not be bankrupt, never have been imprisoned or have violated position or property, not have been evicted from the government or a state enterprise, have maintained the right to vote, and never have been evicted from the fund committee. Committee members can serve a maximum of two years with half of the committee members being replaced each year.

¹⁶According to the sample regulations, committee members were by regulation allowed to divide ten percent of the fund profits among themselves as compensation for their work. Few of the funds surveyed compensated committee members, however.

¹⁷While a general meeting of fund members is required to take place at least once a year, only 85 percent of the funds interviewed reported having these general meetings. The committee plays the primary administrative role in the fund and typically reported meeting one to two times a year to evaluate loan applications.

¹⁸Instruction manuals of accounting procedures were provided by various government agencies. These manuals were roughly 50 pages, and while groups noted that the accounting was tedious, complicated and difficult, none claimed that it was unmanageable.

For lending, the fund was typically divided into two portions: 900,000 baht for investment, and 100,000 baht for an emergency fund.¹⁹ According to the institutional survey, village funds lent out on 950,000 baht in the first year, and according to the household data lending increased about ten percent from the first to the second year. In order to ensure equal access to the funds, regulations stipulated a maximum loan size of 20,000 baht.²⁰ Loans above this amount require approval by all members of the fund, but loans were not supposed to exceed 50,000 baht (about \$1100) regardless. Less than five percent of loans exceeded 20,000 baht, but we do observe four households with loans exceeding 50,000). The repayment period could not be set longer than one year. In addition, villagers claim that they were required to charge a positive rate of interest on loans, and interest rates varied from two to twelve percent, with an average nominal interest rate of seven percent. Another suggested policy that was generally adopted was the use of two guarantors for loans, though the number of guarantors required ranged from one to eight across the sixty-four institutions.²¹ Only eleven of these institutions required collateral, and only three had fully collateralized loans. Repayment was quite high. According to the household data, using a 90-day definition, less than three percent of village fund credit lent out in the first year was in default in the second year.

Committee members typically were to decide who receives loans. The evaluation of the loans included the members' ability to repay, the appropriateness of the investment, and the amount requested. Given the small loan sizes, institutions made a large number of loans, and a large fraction of households received loans. About 55 percent of households received loans from the village fund in our sample.

Seventy percent of the village funds also offered savings services, with most of these requiring that members save and make pledged deposits into their accounts. Members' savings are jointly held in a separate (individual) BAAC savings account. One suggested set of savings regulations that was often followed was that all members must pay an application fee, and buy at least one, but not over 20 percent of shares in the fund. Another suggestion was pledged savings funds with the following policies: deposits are made on a given date, pledged amounts varying from 10 to 500 baht across members, and pledge amounts able to be changed once a year. The average nominal interest rate on savings was just 0.5 percent, that is, a negative real interest rate. The total stock of

¹⁹ Many funds claimed this was a requirement of the program, but again it appeared to only have been an element of the sample village fund regulations.

²⁰ About 35 percent of all loans are of this maximum size.

²¹ Other suggested policies that were often adopted: a late payment penalty of 0.5 percent per day and a duration for emergency loans that was less than one year.

initial savings averaged about 4000 baht across funds. Some funds lent out member savings, while others limited the limited loans to the initial transfer.

2.2 Quasi-Experimental Design of the Program

As described in the introduction, the program design was beneficial for research in two ways. First, it arose from a quick election, after the Thai parliament was dissolved in November, 2000, and was rapidly implemented in 2001. None of the funds had been founded by our 2001 (May) survey date, but by our 2002 survey, each of our 64 village had received and lent funds, lending 950,000 baht on average. Households would not have anticipated the program in earlier years.²² Second, the same amount was given to each village, regardless of the size, so villages with fewer households received more funding per household. Regressions below report a highly significant relationship between household's credit from a village fund and inverse village size in 2002 after the program.

There are strong *a priori* reasons for expecting this variation in inverse village size in the years of the program to be exogenous with respect to important variables of interest.

First, villages are geopolitical units, and villages are divided and redistricted for administrative purposes. These decisions are fairly arbitrary and unpredictable, since the decision processes are driven by conflicting goals of multiple government agencies. (See, for example, Pugenier, 2002 and Arghiros, 2001). Data for the relevant period are unavailable, but between 2002 and 2007 the number of villages increased by three percent, while since 1960 the number of villages increased by roughly 50 percent.

Second, because inverse village size is the variable of interest, the most important variation comes from a comparison among small villages (e.g., between 50 and 250 households). Indeed, we focus our baseline estimates on these villages, but show that results are quite robust to including the whole sample. That is, our analysis is not based on comparing urban areas with rural areas, and we are not picking up the effects of other policies biased toward rural areas and against Bangkok.

Third, village size is neither spatially autocorrelated, nor correlated with underlying geographic features like roads or rivers. Figure 1 shows the random geographical distribution of villages by decile of village size over the four provinces (Chachoengsao, Lopburi, Buriram and Sisaket) in the year 2001. The Moran spatial autocorrelation statistics in these provinces are 0.019 (standard

²²Although villages did received the funds in different months of the year, the precise month that funds were received is uncorrelated with the amount of credit per household after controlling for village size.

error of 0.013), 0.001 (0.014), 0.002 (0.003), and 0.016 (0.003), respectively.²³ Only the Sisaket autocorrelation is statistically significant, and the magnitudes of all of them are quite small. For comparison, the spatial autocorrelation of the daily wage in villages ranges from 0.12 to 0.21. We also checked whether village size was correlated to other underlying geographic features by running separate regressions of village size on distance to nearest two-lane road or river (conditioning on *changwat* dummies). The estimated coefficients were 0.26 (standard error of 0.32) and -0.25 (0.24), so neither was statistically significant. Small villages did tend to be located closer to forest areas however, where the coefficient of 0.35 (0.03) was highly significant, indicating that forest area may limit the size of villages.²⁴ Nonetheless, these regressions explain at most five percent of the variation in village size, so the variation is not well explained by geographic features. We have included roads, rivers, and forest in Figure 1.

Finally, since we control for both village size and household level fixed effects, any contamination would need to result from village size capturing changes in the outcome variables over time, which is doubtful. We verify in Section 3.4 that village size is unrelated to pre-existing differences in levels or trends of the variables that we examine.

2.3 Data

As stated in the introduction, our data are panel survey data from the Townsend Thai dataset.²⁵ We utilize five years (1997-2001) of data before the onset of the program and two years (2002-2003) of post-program data. We focus on two components of the survey (the household data and the institutional data), and supplement the data with information gathered in informal interviews conducted in the field. In addition, we corroborate some of the findings in a parallel monthly longitudinal survey.

The household panel data set is a stratified, clustered, random sample, including 15 households in each of 64 villages distributed across four provinces (*changwats*) of Thailand - the *changwats* of

²³The general formula for Moran's statistic is:

$$I = \frac{n}{\sum_{i=1}^n \sum_{j=1}^n w_{ij}} \left(\frac{\sum_{i=1}^n \sum_{j=1}^n w_{ij} (z_i - \bar{z}) (z_j - \bar{z})}{\sum_{i=1}^n \sum_{j=1}^n (z_i - \bar{z})^2} \right)$$

where n is the number of observations (villages), z_i is the statistic for observation i (village size of village i), and w_{ij} is the weight given villages depending on their spatial distance. Here we use inverse cartesian distance between villages.

²⁴Forest conservation efforts have driven some redistricting decisions but these decisions have been largely haphazard and unsystematic. For discussions, see Pugenier (2001) and Gine (2005).

²⁵See Townsend, et al. (1997).

Chachoengsao and Lopburi in the Central region relatively near Bangkok, and Sisaket and Buriram in the poorer Northeast region.²⁶ The attrition rate from year to year averaged only three percent annually so that, of the 960 households surveyed annually, 800 of them were followed for the full seven years. Attrition was largely due to migration.

The household data set has several strengths. First, it is the only panel data from Thailand that spans across the pre- and post-program years. Second, the data is exceptional in its breadth and level of detail. These data include information on education, assets²⁷ and investment, income and expenditures in production, borrowing and saving through various forms, consumption, occupation, businesses operated, and household composition, for example. Using credit as an example of the detail in the data, for every year we have a record of all loans, both formal and informal, that a household has taken. These loans include the amount of the loan, date of the loan, duration, amount to be repaid, interest rate, lender, stated reason for borrowing²⁸, collateral used, value of collateral, whether the loan has been repaid, and the consequences of defaulting on the loan. The lending environment in these villages is very nuanced, with the BAAC, commercial banks, family, relatives, money lenders, and other quasi-formal village institutions in addition to the village funds all playing significant roles.²⁹

The panel data also include an institutional component which surveyed all of the quasi-formal micro-financing institutions encountered in the survey villages over the seven years of data: the founding; membership, saving, lending, and default policies; and the organizational structure and financial relationships of the institutions. These data are the source of many of the descriptive statistics given above. The survey data also include the record books of the institutions themselves, which is used to compare households' borrowing to institutions' lending in a village. The 64 "million baht" village funds were first in the survey in 2001, but other smaller village funds pre-date the

²⁶The survey design was based in part on the results of prior field research in the Northern region (see Townsend, 1995).

²⁷The initial 1997 value of real assets is found by depreciating the purchase price of the asset (in 1997 baht) from the time of purchase to what it would have been worth six years ago. We assume that the depreciation rate for all household and agricultural assets is 10 percent per year. One exception is land, the value of which we do not depreciate over time.

The retrospective wealth levels are incomplete in (at least) two respects. The first issue is that we only have information on household and agricultural assets that the household still owns. The second concern is that we do not have any information on past financial assets and liabilities. Fortunately, financial assets and liabilities tend to make up a small fraction of current household wealth, and so were probably also a small fraction of past wealth.

Subsequent asset levels were found using current investment data and a depreciation rate of ten percent.

²⁸Variables measuring the amount of credit borrowed for different purposes are based on these reported reasons for borrowing.

²⁹See Kaboski and Townsend (1998)

million baht funds.³⁰

Table 1 gives summary statistics for the relevant variables of the annual household data used in this paper.

The monthly panel is a smaller panel of 400 households in 16 villages that differ from the annual panel data, but are drawn from a common survey in 1997 in the same changwats. Despite the monthly panel being a much smaller sample and creating many issues involving timing because of its high frequency, it has strengths that complement the annual data. In particular, the monthly data includes not only income, but separate records for labor supply (measured in days), which allow for daily wage rates by activity to be calculated.

Finally, we use data from the Community Development Department (CDD), which includes all villages in our provinces, for our geographic analysis.

3 Methods

In dealing with the data, we focused on the effects of village funds on short-term credit (defined as loans of one year or less). The vast majority of village fund credit was short-term, and so we wanted to see its impact on the short-term credit market and abstract away from other credit markets. If long term credit were included, credit variation caused by one household taking out a long-term mortgage on a large home might be too large, swamping any effects of village fund credit that could otherwise be observed.

The dependent variables we focus on are divided into four categories:

- First, we measure the impact of the village fund credit on the short-term credit market, including: its effects on total short-term credit; borrowing from other formal sources (i.e., the BAAC and commercial banks); the stated reasons for borrowing (i.e., business investment, agricultural investment, fertilizer/pesticides, an consumption); and measures of the tightness of credit markets (interest rates, default and informal borrowing).
- Second, we measure the effect of village fund credit on consumption and its different com-

³⁰Consumption is non-durable in that it excludes household asset expenditures, and includes only food, drink, fuel, clothing and services. Consumption is measured by a solicitation of 13 disaggregate items that best predict aggregated non-durable consumption expenditure in the larger more comprehensive SES survey. In practice 50-80% of the variation can be explained by these 13 items. A price index for each of the four provinces was created by the average price of the inter-quartile, 25-75% range of purchases and sales of the key consumption items for which both quantities and values were recorded. Given the weights on each component, impacts on the component of consumption do not simply sum to the total impact (see Table 5).

ponents. Specific components include grains, dairy, meat, fuel, clothes, home repair, vehicle repair, eating out, tobacco, alcohol (consumed both in and out of the home), ceremonies, and education.

- Third, we assess the impact on the income and productive decisions of households. In particular, we look at overall asset and income growth, the sources of both net income (agriculture, business, and wages/salaries), investment (agricultural and business), input use (fertilizer/pesticides), and sources of gross agricultural revenues. We also look at wage rates and days worked.
- Fourth, we look at differential impacts on the above variables in female-headed households. Microcredit is often targeted toward women, and theory (e.g., Bourignon, et al., 1994, Brown-ing and Chiappori, 1998) and evidence (e.g., Pitt and Khandker, 1998, Kaboski and Townsend, 2005) suggest that impacts may differ across men and women.

We propose the following specification for the impact of short-term village fund credit ($VFCR_{n,t}$) of household n at time t on outcome measure $y_{n,t}$:

$$y_{n,t} = \sum_{i=1}^I \alpha_i X_{i,n,t} + \beta VFCR_{n,t} + \phi_t + \phi_n + \gamma invHH_{n,t} + \varepsilon_{n,t} \quad (1)$$

The X_i are a set of household control variables including number of adult males, number of adult females, number of children, a dummy for male head of household, age of household head, age of head squared, years of schooling of head. In addition, we allow for a time-specific fixed-effect ϕ_t , a household-specific fixed-effect ϕ_n , and an effect that potentially varies by the inverse number of households in the village ($invHH_{t,n}$).

Equation (1) has strengths and disadvantages. On the one hand, by not adhering to one particular theoretical model, it allows us to look at a wide range of outcomes that go beyond the predictions of an explicit theory. On the other hand, equation (1) is at best a reduced form attempt to approximate a more explicit behavioral model.³¹ In Kaboski and Townsend (2008), our structural model implies that credit interventions ought to affect the growth rate of income and

³¹We also used the differenced version of equation (1). This specification had advantage of allowing for fixed effects on not only levels, but also changes. The specification produced broadly consistent results, but for the components of consumption and income where measurement error is greater, results were often no longer significant.

asset accumulation, while affecting the level of choice variables such as consumption and investment. (When we focus on specific components of income, we look only at levels, since these measures are noisy, and differencing appears to eliminate most of the signal in the data.) Similarly, for the three outcome variables that may proxy borrower’s *ex post* ability to repay loans, default, interest rates and borrowing from informal sources, we ran alternative regressions using either current village fund credit $VFCR_{n,t}$ or the lagged value of village fund credit, $VFCR_{n,t-1}$.

3.1 Heterogeneity of Impacts

In the theories that motivate our study, unobserved heterogeneity (i.e., ability, project size, permanent income) is important and leads to heterogeneous impacts of exogenous shifts in intermediation (see Kaboski and Townsend, 2008, and Gine and Townsend, 2003, for example). Also, impacts can be non-linear and time-varying. Moreover, general equilibrium impacts may play a role, and so a precise policy-relevant interpretation of β is limited, and we will not assign one. We view estimates of β as rough but nonetheless informative measures of an average linearized impact of the program on village households, scaled into per baht of credit injected terms.

Still, we are interested in potentially observable heterogeneity in impacts. If women are indeed more constrained, female headed households may be differentially impacted by the program. When estimating the differential impacts of female-headed households, we use an additional interaction term of village fund credit with a dummy variable for female headed households:

$$y_{n,t} = \sum_{i=1}^I \alpha_i X_{i,n,t} + \beta_1 VFCR_{n,t} + \beta_2 VFCR_{n,t} * \chi_{female,n} + \phi_t + \phi_n + \gamma invHH_{n,t} + u_{n,t} \quad (2)$$

where $\hat{\beta}_2$ is the differential impact of credit on female-headed households.

We also looked at impacts based on two other potential proxies for the degree a household is constrained: tercile of time-averaged income and land-ownership. Households with higher income tend to borrow more (see Kaboski and Townsend, 2008), so we conjectured that they may be less constrained by the availability of credit. Similarly, land is necessary to collateralize loans (from the BAAC, in particular), and so landowners may have been less constrained. We found no evidence of differential impacts along either of these dimensions, however, and so we do not report the results.

3.2 Estimation

For the actual estimation of the equations above, we use a two-stage instrumental variables approach for village fund credit. The instrument used is the interaction between the inverse number of households in the village and the post-program year dummies. That is, we control for variation across households correlated with the inverse of village size, but use the additional effect of village size in post-program years ($invHH_{t,n} * \chi_{t=t^*}$, where t^* is the relevant program year) as our instrument. This first-stage regression is therefore³²:

$$VFCR_{n,t} = \sum_{i=1}^I \delta_i X_{i,n,t} + \theta_t + \theta_n + \lambda_1 invHH_{t,n} + \lambda_2 invHH_{t,n} \chi_{t=2002} + \lambda_3 invHH_{t,n} \chi_{t=2003} + e_{n,t} \quad (3)$$

The sufficient assumptions for ensuring consistency refer to the error terms in the second-stage (outcome $y_{n,t}$) equations, and are given below:

Orthogonality Assumption:

$$\begin{aligned} \varepsilon_{n,t}, u_{n,t} &\perp invHH_{t,n} * \chi_{t=2002} \\ \varepsilon_{n,t}, u_{n,t} &\perp invHH_{t,n} * \chi_{t=2003} \end{aligned} \quad (4)$$

In the discussion of impacts, we will primarily focus on significance of estimates $\hat{\beta}$ and $\hat{\beta}_2$ in equations (1) and (2), respectively, at the five-percent level, but also point out significance at the ten-percent level, when those results are supported by multiple regressions.

Table 2 gives a sample of the first- and second-stage estimation results from the 2SLS procedure on equations (3) and (1), respectively. The variables of greatest interest are italicized.

In the first stage estimates on the top of the table one can see that the instrument, inverse village size, is strongly predictive of village fund credit in the years of the Million Baht Program, but not otherwise. The t-statistics are 17.2 and 19.1 in 2002 and 2003, respectively. The magnitude

³²The corresponding equation for default and borrowing from informal sources, where lagged credit is important is:

$$VFCR_{n,t-1} = \sum_{i=1}^I \delta_i X_{i,n,t} + \theta_t + \theta_n + \lambda_1 invHH_{t,n} + \lambda_2 invHH_{t,n} \chi_{t=2002} + e_{n,t}$$

$u_{n,t} = \phi_t + \phi_n + \gamma invHH_{t-1,n} + \varepsilon_{n,t}$ and we make the the orthogonality assumption that $\varepsilon_{n,t} \perp invHH_{t-1,n} * \chi_{t-1=2002}$.

of the interacted instrument in 2002 of 575,000 is over 60 percent of the 950,000 (an accumulated flow) that village funds claimed to have lent out on average. The higher coefficient of 645,000 in 2003 is in line with the higher total household borrowing from village funds in 2003. (Both are about ten percent higher.) So the coefficients are both statistically significant and economically meaningful.

The second stage shows that total (i.e., from all sources) village fund credit increased in response to village fund credit, since the $\hat{\beta}$ estimate is 1.36. Notice also that $\hat{\gamma}$ in equation (1) is not a significant predictor in this regression. We return to this in Section 3.4

3.3 Outlier Robustness

The data show a great deal of variability, and so the results can be very sensitive to a single or handful of observations. For example, the vast majority of investments and loans are small, so that one major investment or loan in the regressions can swamp all the activity happening at a smaller scale.

We run seven different regressions in order to deal with this problem:

- The first regression is a standard two-stage fixed-effect least squares regression omitting households in villages with greater than 250 households and fewer than 50 households. This excludes nine of 64 villages. In 2002, the two very small villages had 30 and 34 households, while the large villages had 268, 297, 305, 314, 400, 900, and 3194 households. We refer to this as the baseline regression.
- The second regression is identical to the first regression except that it uses all 64 villages.
- The third regression drops the top and bottom one percent of non-zero values of the dependent variable. If there is a mass point greater than one percent at (at least) one of the endpoints of the distribution, we do not drop any observations from that end.
- The fourth regression deals with outliers by modifying the dependent variable into a dummy variable. Thus, the dependent variable used in this regression are indicator variable $\lambda_{y_{n,t}>0}$, which is one if the dependent variable is positive, and zero otherwise. For example, the agricultural investment variable would be one if the household made an agricultural investment (regardless of the size of the investment) and zero if it did not. The village fund credit in

this regression is similarly an indicator variable, $\chi_{VFCR_{n,t}>0}$. Except for the problems in interpreting coefficients in a linear probability model, the estimated coefficients can be viewed as the increase in the probability that the dependent variable is positive given a household received a loan from the village fund.

- The fifth regression is similar to the fourth regression, except the indicator variable used is whether the dependent variable is above the panel average for the household, $\chi_{y_{n,t}>\bar{y}_n}$ in levels and $\chi_{\Delta y_{n,t}>\overline{\Delta y_n}}$ in differences. That is, the indicator is one if the dependent variable is higher than average for the household, and zero otherwise. The indicator variable for village fund credit is analogous, $\chi_{VFCR_{n,t}>\overline{VFCR_n}}$ or $\chi_{\Delta VFCR_{n,t}>\overline{\Delta VFCR_n}}$. Again, the loose interpretation of the coefficient on village fund credit would be the increase in the probability that the dependent variable is above average given a household had above average credit from village funds.

3.4 Exogeneity of Village Size

The results section will focus on our $\hat{\beta}$ estimates, but here we will first focus on the estimates of $\hat{\gamma}$, the coefficient on inverse village size that is not specific to program years. That is, we ask the question whether inverse village size is a significant explanatory variable even in non-program years.

On the one hand, the analysis of $\hat{\gamma}$ estimates is somewhat limited in how relevant and informative it can be to our exogeneity assumption. Regarding its relevance, our exogeneity assumption requires that *after controlling for inverse village size*, the differential impact of inverse village size in the years of the program be the result of the program. That is, it is effectively a difference-in-differences approach, so $\gamma = 0$ is neither necessary nor sufficient for our exogeneity assumption (4) to hold. Regarding its informativeness, since we include household-specific fixed effects, γ is effectively identified using the relatively small within-village variation in inverse village size over time.

On the other hand, if we had observed that village size were significantly explaining variation in our outcome variables even before the program, then we might have questioned our exogeneity assumption. It is therefore comforting to report that inverse village size generally plays an important role only in program years. In regressions using the 37 variables (i.e., 11 credit market variables, including new short-term credit and short-term village fund credit itself; 14 consumption variables, including total consumption; and 12 income, investment, and input use variables including income

and asset growth), only three are significant even using a more conservative ten percent level of significance. 3 out of 37 is 8 percent, and so even these may simply be Type I errors. Small villages tended to have higher levels of short-term credit in fertilizer ($\hat{\gamma}=1.14e6$ with a standard error of $5.02e5$) and higher shares of total income from rice (8.23, std. error 2.94) and other crops (4.13, std. error 2.20). Thus, they appear more agrarian. Oddly, when we focus on all villages, not just those with between 50 and 250 households, the fertilizer result completely disappears, though the results for rice and other crop income remain.

The possibility of differential trends is perhaps more relevant to our difference-in-difference approach. Since the program occurs during the latter years of the panel, if village size were associated with differential changes over time, this would bring the validity of our instrument into doubt. To analyze this, we modified the previous regressions of the form (1) to:

$$y_{n,t} = \sum_{i=1}^I \alpha_i X_{i,n,t} + \beta VFCR_{n,t} + \phi_t + \phi_n + \gamma_1 invHH_{t,n} + \gamma_2 invHH_{t,n} * t + \varepsilon_{n,t} \quad (5)$$

where γ_2 now measures time trends that vary with the number of households in a village. Here again we found little evidence of differences across villages. The estimates of $\hat{\gamma}_2$ was only significant at the ten percent level in one of the 37 regressions, well within the rate of Type I errors. Smaller villages showed higher growth in the fraction of total income coming from wages ($\hat{\gamma}_2=-1.29$, std. error 0.75), but inclusion of the control only reinforced the estimated *positive* $\hat{\beta}$ impact of village fund credit on the fraction of income that coming from wages.

3.5 GIS Robustness

Another question of interest is to what extent the impacts of credit spillover to non-borrower households. One interpretation of the above specifications assumes that the effects are only on the borrowing household. Of course, viewing each village as a small (open) economy, we might presume that credit injections could affect even non-borrowing villagers, through internal general equilibrium effects, in particular. In this case, a second interpretation of the $\hat{\beta}$ estimates in (1) would be the impact of an additional dollar of credit in the village on the outcome, rather than the impact of directly borrowing an additional dollar on the household's outcome. What is important for this interpretation is that households only benefit from credit injection into its own village. That is, any impacts of credit on non-borrowers must be local to the village.

We test whether it is the local injection of credit into the village that drives our results, or whether neighboring village also has important effects. That is, we construct a GIS control variable for the size of neighboring villages. The control variable is a spatial kernel estimate of the inverse village size (number of households) of neighboring villages (e.g., all villages in a 5 kilometer radius). The second-stage regressions are therefore of the form:

$$y_{n,t} = \sum_{i=1}^I \alpha_i X_{i,n,t} + \beta VFCR_{n,t} + \mu invHH_{n,t,neighborhood} * \chi_{t \geq 2002} + \phi_t + \phi_n + \gamma invHH_{t,n} + \varepsilon_{n,t} \quad (6)$$

The results we present are overwhelmingly robust to the inclusion of such a neighborhood control variable. The signs of $\hat{\beta}$ estimates from regressions of equation (6) agree with those of equation (1) in eighty-eight percent of the regressions, and the magnitudes are generally quite similar. Of course, the reduced independent variation in $VFCR_{n,t}$ often increases the standard errors of estimates, but nevertheless seventy percent of the $\hat{\beta}$ that were significant in equation (1) were also significant in equation (6). Finally, the $\hat{\mu}$ estimate was not a strong predictor of outcomes and was significant in only twenty percent of the regressions. In the Results section that follows, we note any important exceptions.

Together, the robustness of our results to the GIS variable support the claim that in the two years after the program's founding, which we study, impacts remained local to the village, and our view of the experiment on separate village economies appears justified.

4 Results

Table 3 presents estimates of the program's short-term impacts on four key summary variables: credit, consumption, asset growth and income growth. The table reports estimates of β along with standard errors, and significance at the five and ten percent levels is noted. Each of the columns corresponds to a different outcome variable, while the rows correspond to the baseline regression (at the top) and the four alternatives that address the influence of outliers (below).

The first column of estimates indicates that the flow of total new short-term credit increased. That is, the program was successful in increasing overall credit and did not simply crowd out other sources of credit. There actually is some evidence from the levels regression that the credit injection may have had a multiplier effect (i.e., a baht of credit injected by the village fund led to

more than one baht of additional total credit), though none are significantly greater than one at the five percent level.

Similarly, the second column shows substantial and significant increase in consumption levels. Indeed, the estimates suggest that the increased value of consumption is of the same order of magnitude as the credit injection, or even larger with the baseline estimate of an additional 2.03 baht of consumption for every baht of village fund credit injected. The estimate that drop outliers also indicate a large number (1.24).

The third column indicates some evidence that credit lowered the log growth of assets. Recall assets includes the value of physical assets and financial assets (net of loans). The estimates from the dummy variable regressions in the bottom two rows show that credit significantly lowered the probability of households with positive (-0.50) and above average (-0.38) asset growth of households. Although, interpretation of linear probability models is necessarily problematic, the coefficients would indicate that households with loans were 50 percentage points less likely to have positive asset growth (row 4), and 38 percentage points less likely to have asset growth above the household's average asset growth (row 5). Of course, such "households-with-loans" interpretations implicitly assume that the impacts flow only to borrowers.

Using similar interpretations, the fourth column indicates that households were (38 percentage points) more likely to have rising income, and (51 percentage points) more likely to have an increase in income that exceeded the household's average increase.

To summarize, we see a substantial increase in credit on the order of the size of the injection, a comparable, perhaps larger, increase in consumption, and a higher preponderance of low asset growth, and high income growth.

The large increase in credit may be evidence of credit constraints. The large increase in consumption – of similar magnitude, if not larger, than the increase in credit – is a striking finding. A major argument in favor of credit interventions like the Million Baht Program is that the poor in non-intermediated sectors actually have returns to investment that exceed market interest rates and the returns to investment in the financially-intermediated sector.

The observed large increase in consumption might indicate that the returns are actually highest in consumption. Such behavior is quantitatively consistent with Kaboski and Townsend (2008)'s structural buffer stock savings model. In this model, two groups increase consumption: consumption-constrained households with short-term liquidity needs, and households with buffer stocks that are larger than necessary after the credit constraint has been relaxed. The second group can make

consumption growth exceed credit growth, since they increase consumption without actually borrowing.³³ The intermediation and growth explanation is that constraints are binding on investment and input use and the observed income growth may reflect this. The asset growth might then be a result of households with higher future income intertemporally substituting toward present consumption (as in the intermediation and growth models). Finally, even though we focus on non-durable consumption, the increase in consumption may have an investment aspect to it.

To gain more insight into these issues, we analyze each of the impacts (credit, consumption, and income/assets) more closely below.

4.1 Impact on the Credit Market

In Table 4, we delve more deeply into the impacts of the program on the credit market. For the purpose of comparison, the first column reproduces the results for the impact on total new short-term credit of Table 3. The next two columns show the impacts of village fund credit on credit from other formal sources, namely, the BAAC/agricultural cooperatives and commercial banks. In the baseline sample, the first column value of 1.11 indicates that the program has a larger impact on BAAC credit than village fund credit itself. This large number for the BAAC drives the high value (2.13) for total new short-term credit in the baseline sample. The very large numbers for total new short-term credit and credit from the BAAC are particular to the baseline sample, however, since using the full sample of villages or dropping outliers lowers both point estimates and standard errors substantially. Indeed, neither of the regressions using dummy variables even find an impact on BAAC credit. Instead, they find that the frequency of short-term loans from commercial banks actually falls mildly.

The fact that the estimates for the BAAC and commercial banks are highly dependent on the treatment of outliers is consistent with the fact that formal lenders make large but infrequent loans (see Kaboski and Townsend, 1998). Indeed, it emphasizes the advantages of looking at these different cuts of the data.

The middle columns of the table show the impacts on short-term credit categorized by the households' own stated reasons for borrowing. That is the right hand-side variables are the sum of all short-term credit borrowed for a given stated purpose. Common stated reasons for borrowing

³³ Another potential way that the program could impact non-borrowers consumption is through relending to non-borrowers as in Angelucci and De Giorgi, 2006. We do not view such indirect borrowing as an important channel in the Thai context, since we found no substantial or significant increase in household lending to others, whether inside or outside of the village.

include the reasons we report (consumption, fertilizer use, business investment, and agricultural investment), but these are not the only stated reasons. We estimate no significant impact on borrowing for business investment or agricultural investments, but salient effects on the amount of borrowing for which either fertilizer or consumption were the stated reasons for borrowing. Depending on the regression, the top three rows indicate that the increase in fertilizer borrowing is 0.41 to 0.76 baht for every baht of village fund credit borrowed, while the increase in borrowing for consumption is 0.63 to 0.72 baht for every baht borrowed from the village fund. Households are 31 percentage points more likely to borrow for consumption, and 39 percent more likely to borrow more than they borrow on average for consumption.

Clearly, the reason for borrowing should be ambiguous, since money is fungible across uses. We will see, however, that the investment and consumption borrowing patterns are reflected by actual levels of investment and consumption, while fertilizer usage is not. Fertilizer and pesticide usage may simply be a fallback reason that households give for borrowing; in the past, a large share of loans from the BAAC in the past were given for such use, for example.

The final six columns of Table 4 show the effect of the program on other aspects of the credit market: interest rates, default, and informal borrowing. We distinguish between the impact on the credit market in the year the loans were taken, and the impact on the credit market in the year the loans were due. The results indicate that the injection of did not appear to have large effects on the overall credit market. First, the fact that short-term interest rates did not fall (and perhaps even rose slightly) is supporting evidence that households were credit constrained.³⁴ The taking of loans seems to have little effect on default and the use of informal credit. The results for the impact on credit market in the year of repayment provide some evidence of tighter credit markets, however. There is some evidence that more households are in default, and face higher interests rates after borrowing, but they do not appear to be resorting more to informal lenders in the year of repayment.³⁵

³⁴When outliers are excluded from the sample there is an increase in the interest rate. This may be driven somewhat by loans from commercial banks or from informal sources such as money lenders, stores, or neighbors, all of whom charge higher interest rates than relatives and the BAAC. Using the average credit per household of 9600 baht, the row 3 estimates of $1.75\text{e-}6$ (current year) and $3.49\text{e-}6$ (subsequent year) would imply 1.7 and 3.3 percentage point (170 and 330 basis point, respectively) higher interest rates. Over the full period, the average nominal interest rate on short-term credit is 9.2 percent (recall Table 1), so quantitatively the effect is not negligible.

³⁵Kaboski and Townsend (2008) allow impacts to vary over the first and second years and find that indeed default rates are significantly lower in the first year but higher in the second year of the program, which would also be consistent with such an interpretation.

4.2 Impact on Consumption

Table 3 showed a substantial impact on consumption, and Table 4 showed that stated borrowing for consumption increased in a similar fashion. We analyze here the impacts on different components of nondurable consumption in Table 5. Durable consumption showed no significant impacts and are therefore not presented. A first observation from Table 5 is that the consumption of several components of nondurables are unaffected by the credit program. The fact that “necessities” like grain, meat and tobacco do not increase is perhaps not surprising, but other components such as ceremonies, eating out, and educational expenditures are also not significantly affected. Our result of no measured impact on educational expenditures should not be construed as evidence against credit constraints in educational investment, since an increase in the opportunity cost of going to school may have offset the reduced cost from credit constraints.

The components with the largest responses to the credit programs are housing repair and vehicle repair, which are investment-like in the sense that they have a durable aspect to them. Housing repair expenditures are sizable but infrequent, and so do not show up in the regression using dummy variables. The estimate indicates that a baht of village fund credit led to 0.26-1.52 baht of expenditures on household repair and 0.15-0.20 baht on vehicle repair.

Vehicle repair expenditures also increased significantly in frequency with households 25 percentage points more likely to have spent money on vehicle repair, and 21 percentage points more likely to spend more than the average for the household. In addition, the frequency of households with positive expenditures (0.17) and above average expenditures (0.33) on fuel increased. Perhaps this is related to vehicle repairs. To the extent that vehicles are necessary inputs into production or transportation to jobs, such repairs may be investments with high returns rather than consumption. Karlan and Zinman (2008) make a similar argument.

The other components with statistically significant increases are spending on dairy (0.05 to 0.06 baht per baht of credit), alcohol consumed both at home (0.06 to 0.09) and outside of the home (about 0.03), and clothing (0.01).

An earlier version of this paper produced mixed reactions toward the measured increase in consumption among policy makers in Thailand. One intention of the program was to use the funds for productive purposes, so some viewed the increase in consumption (together with the increased default) as evidence that villagers had “wasted” the funds. The breakout of consumption shows however that the components that policy makers might particularly associate with waste (e.g.

alcohol or clothing) show relatively small increases or even declines (e.g., tobacco), while again the repair services, which have an aspect of investment to them, show the largest response.

We turn to now to impacts on traditional productive activities.

4.3 Impact on Productive Activities

Recall that in Table 3, we saw that income growth increased as a result of the village fund credit. Table 6 examines this in more detail by showing impact estimates for income, investment and input use. In the first three columns, we examine the effect of credit on the fraction of income generated from the most important sources of earned income: business profits, wage/salary labor income, and agricultural income from rice, other crops and livestock.

The relative importance of business and wage income increased in response to the program. Given the average credit per household in the data of 9600 baht, the significant coefficient on business income in the second row of $3.00\text{e-}6$ about a three percentage point increase in the fraction of income from business profits. Since business income is less common and is also quite skewed, the treatment of outliers greatly affects the significance of results. The effect on wage and salary income is more robust to the cut of data. The second row coefficient of $5.93\text{e-}6$ indicates that a household with the average credit per household experienced on average roughly a six percentage point increase in its fraction of income from labor income. Similarly, we see declines in the fraction of income from rice and other crops, and an increase in the importance of livestock. Only the decline in the fraction of income from “other crops” is significant, however. It is roughly the order of magnitude of the increase in business income. These results are broadly consistent with the models of intermediation, entrepreneurship, and growth, and the stated aims of the program.

On the other hand, the results in the middle columns on measures of investment and input use do not support a story in which credit is needed for either start-up costs or business investment. Specifically, the last five columns focus on this investment behavior and the use of inputs. We see no significant impact on business starts, and the coefficient on business investment is actually negative. We do however see some evidence of increased payment of wages. This may indicate that set up costs or the fixed cost of investment are less important barriers to business expansion than some models assume (e.g., Banerjee and Newman, 1993, Lloyd-Ellis and Bernhardt, 2000). Indeed, over half of the new businesses started in the seven year panel reported no start-up costs, and 75 percent of new businesses required less than the 9600 baht/household of average credit injected by the program. It may also be that many business options require set-up costs of indivisible

investments that are too large to be reasonably impacted by microfinance interventions (Ahlin and Jiang, 2008, Buera, Kaboski, and Shin, 2008).

Related, the lack of an impact on business investment may also reflect the fact that business investments are infrequent, large and skewed. Only 8 percent of observations show positive business investment, and one percent of observations constitute three-quarters of all business investment. These large investments that drive overall investment levels (e.g., a warehouse or ten-wheel truck) are not only rare, they may be less likely to be affected by the presence of microloans than more common small investments. Indeed, we do discern impacts on agricultural investment, but only a significantly higher probability of investing (22 percent more likely) and of investing more than the household's average (17 percent more likely) when village fund loans are taken. Kaboski and Townsend (2008) develop this argument quantitatively, showing that the infrequency and lumpiness of investments may make it difficult to discern impacts on investment levels given our sample size.

It is also possible that credit plays a more important role as working capital for input use rather than for investment. We see an actual reduction in the use of fertilizer, despite the increase in the amount of borrowing for which fertilizer was a stated reason. The decline in fertilizer usage may be consistent with the observed decline in the fraction of income from crops.³⁶ In contrast, we find some evidence of an increase in payments to workers (0.17 in the third row), which corroborates the increased importance of wage income. It is also interesting that credit did not significantly increase the fraction of households businesses (as can be seen by row 4), or hiring outside workers (not shown). This again may imply that the impact is not running through the extensive margin, but instead the intensive margin. Payments to labor may also be running through the wage (or hours), since the fraction of workers with labor market earnings does not significantly increase.³⁷

An increase in the actual wage rate is a strong prediction of models of intermediation, entrepreneurship and growth, and we therefore examine the evidence for wage rate increases a little more directly. Although the annual data does not have separate data on wages, the monthly panel provides direct evidence of a general equilibrium effect on prices (i.e., wages) from the program. The monthly data breaks out days of labor supply and daily wages by activity, but is a smaller

³⁶Analysis of our soil samples indicates that fertilizer usage may have already been excessive. (Wivutvongvana and Jiraporncharoen, 2002). Village fund credit is more fungible than fertilizer loans from the BAAC, which are denominated in currency, but sometimes distributed in kind. Substitution toward village fund credit could therefore drive down the total amount of fertilizer in the village, even if fertilizer were the stated reason for borrowing for both sources.

³⁷In the annual data, we have indirect measures of wage earnings in villages, namely the responses of maximum wage paid and minimum wage paid for the small sample of households who hired labor. These variables did not pick up significant effects on the wage, but are also very imperfect measures.

sample of (16) villages, and the very high frequency of the data creates timing issues (e.g., should credit affect outcomes in the month it is disbursed, some period after disbursement, or for the loan period, or after it is repaid?) Given the smaller sample of households in the monthly data, we emphasize results with a ten percent level of significance. We do not present the results, but, using regressions that best replicate the annual data, the monthly data corroborates the significant positive impact we found on labor income.³⁸ Thus, we view this data as informative.

Regressions using the monthly data also yield significant wage effects for several occupation categories consistent with the findings on income as shown in Table 7. Results for the dummy variable regressions are not available, so we instead include results for a regression that drops the 5 percent of outliers in the tails.³⁹ There are three interesting findings to note. First, in the first column, the regressions that control for outliers show a significant negative impact on agricultural wage rates, which would be consistent with the lower fraction of agricultural income described above. Second, there are no significant impacts on factory workers, merchants, and government/professional wages, which would often be performed outside of the village. Third, there are significant positive impacts on wages in general-non agricultural work, construction, and “other”, which together constitute a third of all observations. The coefficients are sizable. For example, given the average credit per household in the data of 9600 baht, the coefficient of 0.0005 would constitute about 15 percent of the average wage. The positive effect on construction wages in the villages, which is even more evident and striking when the contemporaneous flow of credit is used instead of the lagged stock, is particularly interesting because it is only evident for local wages. Wages for construction work in other counties (including Bangkok) do not increase. This is consistent with the idea of village economies, with (partially) segmented labor markets, and also with the increases in the consumption of household repairs found above.

4.4 Differential Impact on Women

We examined whether the impacts of credit were significantly different for female-headed households using all of the outcome measures. Overall, perhaps the most surprising result that female-headed households behave similarly to households headed by males. We found no significant differential

³⁸The credit variable is a point in time stock of outstanding short-term credit, while the outcome variables are the twelve month growth in total income and income by source twelve months later.

³⁹The regression using a dummy variable for positive outcome is not possible, since wages are all positive. The regression using a dummy variable for above average outcome is possible, but it is not straightforward whether the household’s average wage for a certain occupation or average overall should be used. Neither of these produced strong results.

impacts of the village fund on female headed household with respect to credit or agricultural income. The only significant differential impacts were on the sources of income, and the distribution of consumption. Table 8 summarizes these impact results, i.e., estimates of $\hat{\beta}_2$ in equation (2).

Looking at the sources of income, the significant difference between male- and female-headed households is that credit causes a relatively larger positive impact on the fraction of female-headed households reporting positive and above average business income than on the fraction of male-headed households. Female-headed households are about ten percentage points more likely to have positive and above average business income (see the fourth and fifth row).

There are also significant responses of female-headed households in their consumption patterns, but not in the ways typically argued in the literature. In other countries, the literature (e.g., Pitt and Khandker, 1998) has found that men tend to spend money on things such as alcohol, while women's spending patterns are directed toward children. Our results in Thailand differ. For example, women do not spend more on children's education in response to credit. Indeed, the credit program significantly lowered the probability that a female-headed household would have educational expenditures above average for the household (see the bottom row). There is also some evidence that female-headed households shift consumption more toward auto repair and clothing, and especially toward meat consumption. Finally, we do find that female-headed households shift consumption less toward alcohol consumed outside of the home, but surprisingly there is some evidence (the last row) that they instead increase their consumption of alcohol in the home. From informal discussions, we have learned that drinking outside of the home is less culturally acceptable for women in Thailand than for men.

5 Conclusions

The Million Baht Village Fund injection of microcredit in villages has had the desired effect of increasing overall credit in the economy. Households have responded by borrowing more, consuming more, and investing in agriculture more often than before. The village fund credit has had the effect of decreasing future assets, increasing future incomes, and making business and market labor more important sources of income. The fact that households increased borrowing despite higher interest rates, together with the consumption, investment, and asset responses point to a relaxation of credit constraints. The increased interest rates, increased credit from non-village fund sources, and increased labor income and especially wage rates indicate important general equilibrium effects that

may have also affected non-borrowers. Such general equilibrium effects from expanded financial intermediation are important channels of development in models of finance and growth.

The large increase in borrowing and consumption and the decline in assets are broadly consistent with buffer stock models of credit constrained households. Our companion paper develops this link more explicitly and in a quantitative fashion, but the reduced form analysis of this paper shows that the composition of consumption increases is not only toward luxury goods but also repairs. Similarly, the increase in income, and the increasing importance of business and labor income are consistent with models of intermediation and growth. The general equilibrium impact on wages that we discover offers more credence to these models, where rising wages play an important role.

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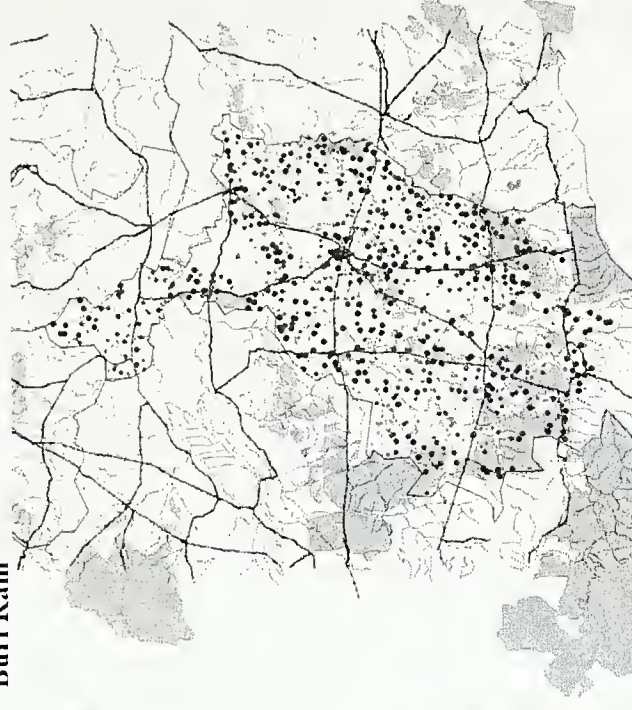
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Number of Households per Villages, Four Provinces, Thailand

Si Sa Ket



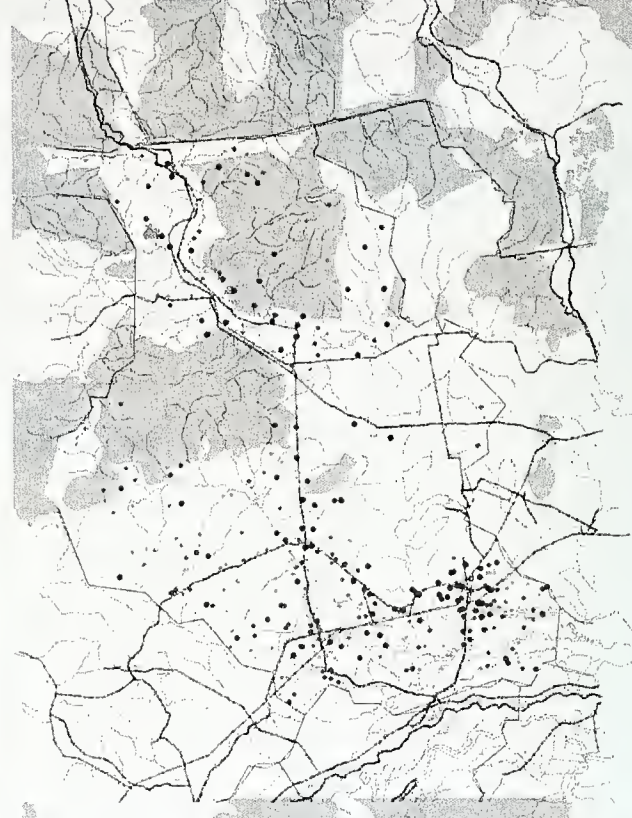
Buri Ram



Chachoengsao



Lop Buri



Number of Households
per Village, CDD 2001

● First Decile

● Second Decile

● Third Decile

● Fourth Decile

● Fifth Decile

● Sixth Decile

● Seventh Decile

● Eighth Decile

● Ninth Decile

Road

Stream

Forest Cover

Table 1. Summary Statistics of Relevant Townsend Thai Household-Level Data

	No of Obs.	Mean	Std Dev
Short-Term Credit Variables			
New Short-Term Credit (Total)	5,831	20,900	50,600
<i>Village Fund Credit</i>	5,831	2,600	6,900
BAAC/Ag Coop Credit	5,831	11,000	30,900
Commercial Bank Credit	5,831	300	7,000
Informal Credit	5,831	5,600	31,800
Credit for Agricultural Investment	5,831	1,400	10,000
Credit for Business Investment	5,831	3,600	31,900
Credit for Fertilizer, Pesticides, etc.	5,831	10,100	33,200
Credit for Consumption	5,831	8,300	24,600
Credit Market Indicators			
Average Short-Term Credit Interest Rate	2,982	0.095	0.139
Flow of Credit into Default	5,831	12,300	75,100
Consumption Variables			
Total Consumption	5,767	75,300	101,500
Education	5,784	5,200	11,000
Grain	5,767	8,900	11,300
Dairy	5,767	2,100	4,400
Meat	5,767	4,100	4,700
Alcohol at Home	5,767	1,900	4,800
Alcohol Out of home	5,767	900	3,600
Fuel	5,767	5,000	11,400
Tobacco	5,767	1,100	3,000
Ceremony	5,767	5,200	13,000
House Repair	5,784	6,300	37,000
Vehicle Repair	5,784	2,100	8,100
Clothes	5,784	1,500	2,500
Eating Out	5,784	1,900	5,400
Income and Asset Variables			
(Total) Net Income	5,825	96,900	193,500
Business Income	5,825	16,500	148,600
Wage and Salary Income	5,808	31,500	65,000
Gross Income from Rice Farming	5,808	20,800	37,000
Gross Income from Other Crops	5,808	21,200	95,100
Gross Income from Livestock	5,808	6,956	50,600
Gross Assets (incl. savings)	5,614	1,577,000	4,108,000
Investment and Input Uses Variables			
Number of New Businesses	5,823	0.05	0.24
Business Investment	5,831	3,400	48,400
Agricultural Investment	5,824	3,300	28,600
Expenditure on Fertilizer, Pesticides, etc.	5,825	9,100	20,700
Other Control Variables			
Male Head of Household Dummy	5,790	0.73	0.44
Age of Head	5,790	53.7	13.4
Years of Education of Head	5,679	6.15	3.17
Number of Male Adults in Household	5,790	1.45	0.90
Number of Female Adults in Household	5,790	1.56	0.76
Number of Kids in Household	5,790	1.54	1.20
Farming Dummy for Household Head's Primary Occupation	5,831	0.61	0.49
Instrument			
<i>Inverse Village Size</i>	5,831	0.011	0.007

Table 2. Sample Regression – Two-Stage Household Fixed-Effect Estimate of the Impact of Current Level of Village Fund Credit on New Short-Term Credit Level

First Stage: Village Fund Credit on Instruments	Coeff.	Std. Err.	t-value
Constant (1997 Dummy Excluded)	-6,400**	2,800	-2.61
Year=1998 Dummy	40	310	0.11
Year=1999 Dummy	90	320	0.27
Year=2000 Dummy	40	330	0.12
Year=2001 Dummy	140	340	0.40
Year=2002 Dummy	2,670**	490	5.48
Year=2003 Dummy	3,090**	500	6.24
Number of Adult Males in Household	-80	150	-0.57
Number of Adult Females in Household	520**	160	3.11
Number of Children (< 18 years) in Household	220**	110	2.00
Male Head of Household	960**	480	2.01
Head of Household's Primary Occupation is Farming	-17	230	-0.08
Age of Head	250**	100	2.50
Age of Head Squared	-2.39**	0.88	-2.73
Years of Education – Head of Household	-1.64	70	-0.02
Inverse Village Size (invHH)	-30,300	48,900	-0.62
Interaction of Inverse Village Size and Year=2002 Dummy	575,300**	33,400	17.2
Interaction of Inverse Village Size and Year=2003 Dummy	645,600**	33,800	19.1
Number of Observations/Groups	5,679 / 828		
Second Stage: New Short-Term Credit on Predicted Village Fund Credit			
Constant (1997 Dummy Excluded)	20,500	22,170	0.18
Year=1998 Dummy	7,230**	2,520	2.86
Year=1999 Dummy	8,420**	2,590	3.25
Year=2000 Dummy	5,220**	2,650	1.97
Year=2001 Dummy	7,160**	2,730	2.62
Year=2002 Dummy	1,260	3,870	0.32
Year=2003 Dummy	810	4,190	0.10
Number of Adult Males in Household	2,240**	1,130	1.99
Number of Adult Females in Household	1380	1,280	1.07
Number of Children (< 18 years) in Household	830	880	0.95
Male Head of Household	11,480**	3,730	3.08
Head of Household's Primary Occupation is Farming	-2980	1,780	-1.68
Age of Head	20	780	0.02
Age of Head Squared	0.08	6.91	-0.01
Years of Education – Head of Household	-470	570	-0.82
Inverse Village Size (invHH)	-153,700	382,500	-0.40
Village Fund Credit (predicted)	1.36**	0.33	4.12
Number of Observations/Groups	5,472 / 800		

Note: ** indicates significance at 5%

Table 3. Summary: The Impact of Village Fund Credit

Response Variable Technique	New Short-Term Credit Level	Consumption Level	Asset Growth Rate	Net Income Growth Rate
Baseline Regression: Only Villages With 50-200 Households	2.13** (0.58)	2.03** (0.81)	-3.52e-6 (2.20-5)	8.1e-5** (3.37e-6)
Regression using All Villages	1.36** (0.33)	2.00** (0.66)	-2.04-5** (9.69e-6)	2.05e-5* (1.23e-5)
Regression without 1% Outliers	1.00** (0.22)	1.24** (0.39)	-8.15e-6 (7.96-6)	1.64e-5 (1.04e-6)
Regression with DVs for Positive Response Value and Village Fund Credit	0.31** (0.11)	‡	-0.50** (0.19)	0.38** (0.19)
Regression with DVs for Above Average Response Value and VF Credit	0.72** (0.12)	0.51** (0.14)	-0.38** (0.19)	0.51** (0.20)

** Significant at 5% level * Significant at 10% level

The independent variables are year dummies, household fixed effect dummies, male head of household dummy, number of adult males, number of adult females, number of kids, age of head and age of head squared, years of schooling of head, gross assets and gross assets squared, income, and inverse number of households in village. The treatment variable is the level of short-term village fund credit. The additional instruments in the first-stage are the inverse village size interacted with a dummy variable for year=2002 and year=2003.

‡ Regression could not be run because all values were positive.

Table 4. Impact of Village Fund Credit on Other Credit, Interest Rate, and Default

Response Variable Technique	New Short-Term Credit	Other Formal Credit		Stated Reasons for Borrowing				Credit Market Indicators			
								Year Borrowing		Year After Borrowing	
		BAAC/Ag. Coop Credit	Commercial Bank Credit	Credit for Agricultural Investment	Credit for Business Investment	Credit for Fert., Pest., etc.	Credit for Consumption	Avg. Short-Term Credit Interest Rate	Amount of Short-Term Credit in Default	Informal Credit	Informal Credit†
Baseline Regression: Only Villages With 50-200 Households	2.13** (0.58)	1.11** (0.39)	0.10 (0.10)	0.11 (0.15)	0.38 (0.37)	0.76** (0.39)	0.63** (0.32)	-1.28e-6 (2.11e-6)	1.01 (1.26)	-0.24 (0.37)	-0.44 (0.85)
Regression using All Villages	1.36** (0.33)	0.47** (0.22)	0.00 (0.06)	0.02 (0.08)	0.10 (0.20)	0.63** (0.23)	0.71** (0.18)	1.52e-6 (1.28e-6)	0.30 (0.76)	-0.14 (0.23)	-0.11 (0.30)
Regression without 1% Outliers	1.00** (0.22)	0.27* (0.16)	-0.04 (0.03)	0.04 (0.06)	0.04 (0.16)	0.41** (0.16)	0.72** (0.13)	1.75e-6* (1.09e-6)	-0.06 (0.30)	-0.10 (0.16)	-0.11 (0.22)
Regression with DVs for Positive Response Value and Village Fund Credit	0.31** (0.11)	-0.03 (0.09)	-0.05** (0.02)	0.08 (0.06)	0.03 (0.05)	0.10 (0.10)	0.31** (0.11)	-0.00 (0.20)	-0.04 (0.13)	-0.06 (0.11)	0.10 (0.13)
Regression with DVs for Above Average Response Value and VF Credit	0.72** (0.12)	0.14 (0.09)	-0.05** (0.02)	0.05 (0.05)	0.07 (0.05)	0.20** (0.10)	0.39** (0.11)	-0.22 (0.28)	-0.02 (0.12)	0.14 (0.12)	0.14 (0.12)

** Significant at 5% level

* Significant at 10% level

The independent variables are year dummies, household fixed effect dummies, male head of household dummy, number of adult males, number of adult females, number of kids, age of head and age of head squared, years of schooling of head, gross assets and gross assets squared, income, and inverse number of households in village. The treatment variable is the level of short-term village fund credit. The additional instruments in the first-stage are the inverse village size interacted with a dummy variable for year=2002 and year=2003. The fertilizer credit regressions also contain the area of cultivated land as an explanatory variable. Standard errors for the binomial regressions are not corrected for heteroskedasticity.

† Regressions are based on specification (3), where the treatment variable is the level of *lagged* village credit.

Table 5. Impact of Village Fund Credit on Consumption and its Components

Response Variable Technique	Components of Consumption													
	Total	Education	Grain	Dairy	Meat	Alcohol Home	Alcohol Out	Fuel	Tobacco	Ceremony	House Repair	Vehicle Repair	Clothes	Eating Out
Baseline Regression: Only Villages With 50- 200 Households	2.03** (0.81)	0.16 (0.10)	0.01 (0.15)	0.03 (0.05)	0.04 (0.04)	0.09** (0.04)	0.03 (0.03)	-0.11 (0.10)	0.02 (0.02)	0.16 (0.12)	1.52** (0.53)	0.20** (0.10)	0.00 (0.02)	0.05 (0.05)
Regression using All Villages	2.00** (0.66)	-0.06 (0.06)	0.03 (0.09)	0.06** (0.03)	0.03 (0.03)	0.03 (0.03)	0.03 (0.02)	0.06 (0.08)	0.02 (0.02)	-0.04 (0.11)	0.76** (0.30)	0.15** (0.06)	0.02 (0.01)	0.00 (0.03)
Regression without 1% Outliers	1.24** (0.39)	0.03 (0.05)	0.02 (0.03)	0.05* (0.02)	0.02 (0.02)	0.06** (0.02)	0.03** (0.02)	0.05 (0.04)	0.01 (0.01)	-0.06 (0.04)	0.26* (0.16)	0.04 (0.03)	0.02** (0.01)	-0.01 (0.02)
Regression with DV's for Positive Response Value and Village Fund Credit	‡	-0.06 (0.08)	-0.00 (0.01)	0.12 (0.11)	0.01 (0.05)	0.19* (0.10)	0.21** (0.10)	0.17** (0.07)	-0.14* (0.09)	-0.04 (0.05)	0.06 (0.11)	0.25** (0.10)	-0.03 (0.09)	0.06 (0.10)
Regression with DV's for Above Average Response Value and VF Credit	0.51** (0.14)	-0.04 (0.13)	0.09 (0.13)	0.11 (0.12)	-0.05 (0.14)	0.22** (0.11)	0.19* (0.10)	0.33** (0.12)	-0.06 (0.12)	-0.09 (0.13)	0.16 (0.10)	0.21* (0.12)	0.21 (0.13)	0.10 (0.13)

** Significant at 5% level * Significant at 10% level

The independent variables are year dummies, household fixed effect dummies, male head of household dummy, number of adult males, number of adult females, number of kids, age of head and age of head squared, years of schooling of head, gross assets and gross assets squared, income, and inverse number of households in village. The treatment variable is the change in short-term village fund credit. The additional instruments in the first-stage are the inverse village size interacted with a dummy variable for year=2002 and year=2003.

‡ Regression could not be run because all values were positive.

Table 6. Impact of Village Fund Credit on Productive Activities

Response Variable Technique	Share of Gross Income					Investment and Input Uses				
	Business Profits	Wage and Salary	Rice Farming	Other Crops	Livestock	Number of New Businesses	Business Investment	Agricultural Investment	Total Wages Paid	Fert., Pest., etc. Expenditures
Baseline Regression: Only Villages With 50-200 Households	1.15e-6 (3.62e-6)	1.02e-5* (5.43e-6)	-5.86e-6 (4.92e-6)	-1.08e-5** (3.69e-6)	3.33e-6 (3.48e-6)	4.85e-6 (3.50e-6)	-0.33 (0.62)	-0.04 (0.42)	-0.21 (0.21)	-0.09 (0.07)
Regression using All Villages	3.00e-6** (1.41e-6)	5.93e-6** (2.10e-6)	-2.13e-6 (1.79e-6)	-3.64e-6** (1.39e-6)	1.69e-6 (1.29e-7)	8.76e-7 (1.98e-6)	-0.14 (0.34)	-0.16 (0.23)	-0.12 (0.33)	-0.32** (0.13)
Regression without 1% Outliers	1.29e-6 (1.00e-6)	6.14e-6** (2.48e-6)	-1.85e-6 (1.76e-6)	-3.57e-6** (1.38e-6)	1.76e-6 (1.23e-6)	8.74e-7 (1.90e-6)	-0.01 (0.15)	-0.06 (0.15)	0.17* (0.09)	-0.09 (0.07)
Regression with DVs for Positive Response Value and Village Fund Credit	0.08 (0.10)	0.10 (0.13)	-0.10 (0.09)	-0.25** (0.11)	0.09 (0.14)	0.04 (0.06)	-0.00 (0.06)	0.15** (0.07)	0.09 (0.10)	-0.11 (0.07)
Regression with DVs for Above Average Response Value and VF Credit	0.12 (0.11)	0.18 (0.17)	-0.21 (0.16)	-0.20 (0.13)	0.13 (0.14)	0.03 (0.06)	0.03 (0.06)	0.12* (0.07)	0.15 (0.12)	-0.25** (0.13)

** Significant at 5% level

* Significant at 10% level

The independent variables are year dummies, household fixed effect dummies, male head of household dummy, number of adult males, number of adult females, number of kids, age of head and age of head squared, years of schooling of head, and inverse number of households in village. The treatment variable is the level of short-term village fund credit. The additional instruments in the first-stage are the inverse village size interacted with a dummy variable for year=2002 and year=2003. The fertilizer expenditure regressions also contain the area of cultivated land as an explanatory variable. Standard errors for the binomial regressions are not corrected for heteroskedasticity.

†† Outliers could not be eliminated because of large mass points (i.e., either >5% or >1%, respectively) at the boundaries of the empirical distribution.

Table 7. Impact on Wages in the Monthly Panel

Response Variable Technique	Wage Rates by Occupation							
	Agriculture	Factory	Merchant	Govt. and Prof.	General Non-Agric.	Constr. Within Village	Constr. Outside County	Other
Number of Observations	2313	2069	109	3101	934	311	119	2605
Baseline Regression	-0.0011 (0.0007)	-0.0001 (0.0001)	-0.0002 (0.0002)	-0.0004 (0.0007)	-0.0005 (0.0006)	0.0001 (0.0009)	-0.0001 (0.0006)	0.0005** (0.0003)
Regression without 1% Outliers	-0.0007* (0.0004)	-0.0000 (0.0000)	-0.0001 (0.0002)	-0.0001 (0.0003)	0.0005* (0.0003)	0.0003 (0.0002)	-0.0001 (0.0006)	0.0002 (0.0002)
Regression without 5% Outliers	-0.0002* (0.0001)	-0.0001 (0.0001)	0.0003 (0.0002)	-0.0001 (0.0002)	0.0005** (0.0002)	0.0003* (0.0002)	0.0000 (0.0003)	0.0001 (0.0001)

** Significant at 5% level * Significant at 10% level

The independent variables are year dummies, household fixed effect dummies, male head of household dummy, number of adult males, number of adult females, number of kids, age of head and age of head squared, years of schooling of head, and inverse number of households in village. The treatment variable is the 12-month-lagged stock of short-term village fund credit. The additional instruments in the first-stage are the inverse village size interacted with dummy variables for months after the fund was started.

Table 8. Differential Impact of Village Fund Credit on Income Sources and Consumption Components of Female-Head Household

Response Variable Technique	Net Income		Components of Consumption						
	Business Profits	Wage and Salary	Education	Meat	Alcohol Home	Alcohol Out	House Repair	Vehicle Repair	Clothes
Baseline Regression: Only Villages With 50-200 Households	3.95e-7 (1.41e-6)	-1.51e-6 (2.12e-6)	-0.01 (0.06)	0.05* (0.03)	0.03 (0.03)	-0.05** (0.02)	-0.01 (0.34)	0.01 (0.06)	0.02 (0.01)
Regression using All Villages	2.92e-7 (1.21e-6)	-1.68e-6 (1.81e-6)	0.02 (0.06)	0.07** (0.03)	0.03 (0.03)	-0.04** (0.02)	-0.13 (0.27)	0.05 (0.06)	0.03** (0.01)
Regression without 1% Outliers	1.20e-6 (1.20e-6)	1.53e-6 (2.11e-6)	0.01 (0.05)	0.05** (0.02)	0.03 (0.02)	-0.03** (0.01)	0.33** (0.16)	0.02 (0.03)	0.02* (0.01)
Regression with DVs for Positive Response Value and Village Fund Credit	0.10** (0.05)	-0.00 (0.06)	-0.02 (0.04)	-0.03 (0.02)	0.07 (0.05)	-0.11** (0.05)	-0.07 (0.05)	-0.06 (0.05)	-0.03 (0.04)
Regression with DVs for Above Average Response Value and VF Credit	0.10** (0.05)	-0.01 (0.08)	-0.12** (0.06)	0.15** (0.06)	0.11** (0.06)	-0.23** (0.10)	-0.05 (0.05)	0.04 (0.06)	0.01 (0.06)

** Significant at 5% level * Significant at 10% level

The independent variables are year dummies, household fixed effect dummies, male head of household dummy, number of adult males, number of adult females, number of kids, age of head and age of head squared, years of schooling of head, and inverse number of households in village. The treatment variable is the change in short-term village fund credit. The additional instruments in the first-stage are the inverse village size interacted with a dummy variable for year=2002 and year=2003.